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Biogeosciences Discussions

Interactive comment on "The effect of resource history on the functioning of soil microbial communities is maintained across time" *by* A. D. Keiser et al.

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We appreciate the positive comments from Anonymous Referee #1 concerning the novelty of our approach and the questions we asked. The referee had eight suggestions for additional discussion points. We have responded to each in full as detailed below.

1) The reviewer is correct: it is possible that some microfauna passed through the 2-mm sieve and were included in the microcosms. We did not observe microarthropods or nematodes, and as such, did not enumerate nematodes and Protozoa that would not be visible under a dissecting scope. We now mention in the discussion that mi-

crofauna could be contributing to the patterns observed. As for explaining patterns observed with site B in Fig 1, we think this is unlikely to be related to microfauna presence/absence because our inoculum approach was common across all experimental units. Consequently, it would seem that all experimental units had an equal chance of receiving fauna. The most plausible explanation is "home-field advantage": please see the response to comment #2 immediately below.

2) The reviewer is correct: in the first of the three rounds site B was distinct. This is support for the "home-field advantage" hypothesis. Although we discuss this hypothesis in the Discussion with relation to increase in function across an experimental history, this is also the most plausible explanation for the high function of inoculum B after round 1 (Figure 1). We now specifically highlight this point in the Discussion.

3) We are unsure if the reviewer is referring to the phylogenetic analysis or mineralization assays. We believe they are referring to the mineralization assays. The fact that we found no statistical support for convergence suggests that overall variance across inocula did not significantly change across rounds. However, we calculated coefficients of variation (which accounts for the increase in the mean across rounds) by round and litter type. There was a decline in the coefficients of variation across rounds. Obviously one cannot test this decrease with a statistical model; however we did conduct a paired t-test of the variances between rounds 1 and 3 that suggested this decline was not statistically significant. This supports our primary statistical models given in the paper – which are the most robust. If the Editor feels that such post-hoc analyses as the paired t-tests are required, we're happy to incorporate them into the paper.

4) The experimental design was based on the one used by Strickland et al. (2009a; 2009b). As in our study, 1 g of litter was combined with 0.5 g of soil. This does mean the soil composes 33% of the total starting mass. Yet because of the difference in mass density of mineral soil and organic material such as litter, the volume of soil was a much small percentage of the overall volume. Specifically, the litter volume in the tubes was $\sim$4 mL and soil <1 mL meaning the environmental volume was primarily

litter material. It was not exhausted over the 100 d of our experiment and, in the study by Strickland et al. (2009a), CO2 was consistently produced from the litters across 300 days (200 days longer than our 100 day-long rounds). We now address these points in the methods to emphasize the suitability of our approach.

5) We agree that plant materials will be affected by sterilization. We could not find peer-reviewed literature describing the effects of autoclaving leaf litter and the alternative (gamma irradiation) is neither cheap nor 100% effective. In short, all sterilization techniques have caveats and the important point the reviewer makes is how this might affect our data interpretation. For our first hypothesis we think not at all as our question related to inocula effects within a litter type; hence differences between litters were not pertinent to the question. For our second hypothesis, if autoclaving reduced differences in litter quality between our two litter environments then we would have been less likely to find support for ideas relating to functional breadth. However, we noted pronounced differences in function arising through litter history (Figure 4), which means our litters must have still be substantially different in quality. If anything then, our design underestimates the effects of litter history and we now mention in the Discussion that our effects might have been even stronger had we used a different sterilization technique. Either way, our litter qualities were different enough to differentiate effects of history on the contemporary function of the microbial communities.

6) This is an important question. Bacterial phyla have been classified into copiotrophic and oligotrophic taxa (akin to the r vs. K life history strategies used to categorize plant/animal taxa). It is thought that Acidobacteria are generally oligotrophic, or K strategists, which have slower growth rates, but can outcompete r strategists in low nutrient environments. Bacteroidetes and Betaproteobacteria are generally copiotrophic, or r strategists, which have fast growth rates and are generally found using labile organic carbon pools (Fierer et al., 2007). Alpha- and Gammaproteobacteria are also likely to exhibit copiotrophic characteristics (Fierer et al., 2007); however, the overall abundances within Alphaproteobacteria, as well as Actinobacteria and Firmicutes,

do not follow in a predictable manner as to allow for broad characterization as one strategist vs. another. Overall, it is interesting to note that the phyla shown in Table 1 were dominated by r strategists, which would be expected in a more organic, nutrient rich, litter environment. It appears that our results are consistent with field-based litter decomposition experiments, where the litter is dominated by copiotrophic phyla, or r strategists (e.g. Pascault et al., 2010). We are not familiar with a similar classification system for fungal communities. We now mention this in the Discussion.

7) We agree. If the experiment ran for longer than 100 days per round, we would expect to find a different community because the litter would be more degraded (McGuire and Treseder, 2010; Schimel, 1995). We now mention this in the Discussion and highlight that it would be very unlikely to influence the conclusions of our study (see response #2 to Reviewer 3).

8) We appreciate the suggestion. We now suggest in the Discussion that there is a need to disentangle whether the differences – or at least a component of the difference – arises from differences in the taxa present and/or the colonization rates of common taxa.

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